Historic Contribution of Sierra Nevada (SN) Snow Pack Runoff to Iron (Fe) limited Upwelling Events off the Golden Gate.

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Public Comments

No public comments were received for this proposal.

Technical Synthesis Panel Review

Proposal Title

#0128: Historic Contribution of Sierra Nevada (SN) Snow Pack Runoff to Iron (Fe) limited Upwelling Events off the Golden Gate.

Final Panel Rating
inadequate

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

The project intends to use diatoms obtained from sediment cores as evidence for iron enrichment to the central California upwelling system with runoff generated in the Sierra Nevada and to detect potential modifications of the runoff by water management. The ecological linkages implicit in the approach are weak since multiple factors influence diatom species composition. No calculations of the magnitude of the iron inputs required to be of ecological significance are provided. No explanation of specific diatom indicators of iron limitation is provided; moreover, nutrient ratios are well known to influence diatom species composition. A pilot study would be more appropriate instead of collection of numerous cores as a first step. The project seems of little relevance to primary CALFED needs.

Additional Comments:

The entire proposal is based on (the presumption of) iron limitation within California coastal waters throughout the last several centuries and the subsequent introduction of iron within surface flows derived from Sierra Nevada snow melt. The PI has proposed a study justified (based on) current knowledge concerning coastal phytoplankton structure and function,

upwelling, and nutrient dynamics. However, without a citation of previous studies presenting evidence for and/or inclusion of data indicating the introduction of iron−enriched riverine flows '…beyond the Golden Gate into the near−shore environment…', any attributing alteration in diatom assemblages within sediment cores to fluctuations in iron is conjecture. Moreover, delineating iron−induced alterations in assemblages from alterations potentially arising from prolonged regional/global climate change across decades can only be inferred, not tested. The conceptual model is confusing. The proposal argues that large changes in runoff characteristics have occurred since arrival of Europeans due to deforestation, hydrologic mining and urbanization, all of which potentially may have increased transport of iron through the Golden Gate into coastal waters. Conversely, management of the Sacramento/San Joaquin River flows during spring snow pack melt would have reduced iron outflows. Though not stated, the first would lead to increased production in coastal waters while the second would decrease production. However, the proposal argues that this effect would be restricted to primarily the segment of shore between the Golden Gate and Pt Ano Neuvo. These sites show little indication of iron limitation today even with the current water management practices. In other words, if more iron does not increase production in these coastal waters today, why should we expect that it would in the past. The main approach is to obtain sediment cores from a number of sites in the Golden Gate region south to Big Sur, date these sediment strata with 14C and 210Pb, and enumerate and identify the diatom frustules (a proxy for production). Climate and anthropogenic effects in these patterns will be identified and removed in order to determine whether natural fluctuations in Sierra Nevada outflow affects coastal productivity. However, the uncertainties with quantifying climate and anthropogenic effects will be large, raising question about the confidence that could be placed in the resulting, and comparatively small, "natural" pattern? The Big Sur site, well removed from the outflow region, will be used as a "control". Yet diatom production here is comparatively low, raising question of whether it can provide the sensitivity needed to serve as an effective control. The success/failure of this project lies

almost entirely with accurate identification/interpretation of the diatom assemblages within the sediment cores. Based on information presented within the attached resume, the PI does not appear to possess any previous experience and/or skill with diatom characterization. A perusal of Barron's publications within annual listings of USGS publications since 1997 indicates a paucity of peer−reviewed manuscripts addressing coastal diatom paleoecology and stratigraphy. Web searches failed to uncover any significant taxonomic work in any recognized diatom− and/or phycological−oriented journals. Moreover, the PI indicates that diatom characterization will be based on image acquisition and recognition technology, − does such automated image processing exists within Barron's laboratory? No previous experience with automated image−recognition methodologies and/or reliability of this methodology for characterizing California coastal diatoms assemblages is presented.

The project intends to use diatoms obtained from sediment cores as evidence for iron enrichment to the central California upwelling system with runoff generated in the Sierra Nevada and to detect potential modifications of the runoff by water management. The ecological linkages implicit in the approach are weak since multiple factors influence diatom species composition. No calculations of the magnitude of the iron inputs required to be of ecological significance are provided. No explanation of specific diatom indicators of iron limitation is provided; moreover, nutrient ratios are well known to influence diatom species composition. A pilot study would be more appropriate instead of collection of numerous cores as a first step. The project seems of little relevance to primary CALFED needs.

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

External reviewers identified substantial technical deficiencies in this proposal. The authors did not establish the link between iron deficiency and limitations on diatom

communities. One of the external reviews questioned the research team’s qualifications in the realm of diatom identification, calling into question their ability to execute this project. Also, this topic area, though interesting, is of only marginal relevance to CBDA’s management questions and objectives.

Technical Review #1

proposal title: Historic Contribution of Sierra Nevada (SN) Snow Pack Runoff to Iron (Fe) limited Upwelling Events off the Golden Gate.

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The goals and objectives of the proposed research are stated clearly. The hypotheses are for the most part well structured and testable. One exception is Hypothesis 7, which is not a hypothesis but a task. The question of iron availability effects on coastal primary production is timely and currently attracting scientific interest. While most of these research efforts have been directed to offshore waters, the role of iron in nearshore waters is less well studied. A large portion of the nearshore work in fact has been done in coastal waters off central California, so the proposed work has a strong backdrop of published studies on which to compare findings. However, while I give strong marks for the central issue here, namely quantifying possible impacts from human activities on primary production in coastal upwelling waters, the proposed research plan as stated in the specific goals is weak.**
Rating	**good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The issue of iron effects on coastal phytoplankton production in this region is well established and the PI's do a good job summarizing this background. Their conceptual model however is a bit confusing. They argue that large changes in runoff characteristics have occurred since arrival of Europeans due to deforestaton, hydrologic mining and urbanization, all of which potentially may have increased transport of iron through the Golden Gate into coastal waters. Conversely, management of the Sacramento/San Joaquin River (S/SJR) flows during spring snow pack melt would have reduced iron outflows. Though they do not state it, the first would by their hypothesis lead to increased production in coastal waters while the second would decrease production. However, they argue that this effect would be restricted to primarily the segment of shore between the Golden Gate and Pt Ano Neuvo. These sites show little indication of iron limitation today even with the current water management practices. In other words, if more iron doesn't increase production in these coastal waters today, why should we expect that it would in the past? I suppose one might argue that it is the residuals of the very high outflows of fine muds generated by hydrologic mining in the last century that is supporting today's high production (that is, resuspension and reworking of these sediments), but that is not what the PI's suggest here. Overall, the conceptual basis for the work seems weak.**
Rating	**fair**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The main approach the PI's intend to take is to take sediment core samples from a number of**

	sites along from the Golden Gate region south to Big Sur, date these sediment strata with 14C and 20Pb, and enumerate and identify the diatom frustules (a proxy for production). They plan to identify and remove climate and anthropogenic effects in these patterns in order to determine whether natural fluctuations in SN outflow affects coastal productivity. However, the uncertainties with quantifying climate and anthropogenic effects will be large, raising question about the confidence that could be placed in the resulting, and comparatively small, "natural" pattern? They intend to use the Big Sur site, well removed from the outflow region, as a "control". Yet diatom production here is comparatively low, raising question of whether it can provide the sensitivity needed to serve as an effective control. I would have liked to have seen some discussion on this point. Finally, the issue of diatom speciation shifts will be of questionable value, given the narrow focus here. There simply will not be enough evidence and understanding to meaningfully interpret the patterns observed with respect to the central hypothesis. **If the goal is really to determine the effects of the outflow of iron through the Golden Gate on coastal production, why not look at contemporary patterns of outflow magnitudes in relation to coastal production? It would simplify the project considerably.**
Rating	**fair**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**In my view, based on the explanations given here I see a low likelihood for success given the planned approaches. However, I think that it is feasible to address the central hypothesis with other approaches.**
Rating	**fair**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**There is no monitoriing component to this proposal**
Rating	**not applicable**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**The product of a successful outcome of this project would be a better understanding of the human influences on coastal productivity in this region. It is unlikely that this information will benefit significantly the S/SJR water management strategies.**
Rating	**good**

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**The project team appears to be well versed in the planned sampling and analyses. They either have, or have arranged for, the necessary scientific infrastructure to conduct the planned studies.**
Rating	**very good**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**I do not have much previous experience with this program, but the requested funds are extremely high for the proposed work in comparison to a NSF scale project. Some of this difference is due to the vessel charter costs, but the cost seems very high even with that removed.**
Rating	**fair**

Overall

Provide a brief explanation of your summary rating.

Comments	**Overall, I believe the PI’s have identified a timely and important issue. Understanding the effects of anthropogenically induced changes in iron supply to coastal waters is a novel concept and the PI’s should be given high marks for its originality. However, the planned experimental approach seems flawed, and the underlying conceptual framework weak. It is with regret that I cannot give the proposal a higher ranking.**
Rating	**fair**

Technical Review #2

proposal title: Historic Contribution of Sierra Nevada (SN) Snow Pack Runoff to Iron (Fe) limited Upwelling Events off the Golden Gate.

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The PI has written a very compelling proposal addressing the utilization of diatom paleoecology for (presumptive) evidence of iron-depletion/repletion within coastal waters, as caused by differential inflows throughout the past centuries, resulting in part, from cultural growth of California. I do agree with the PI's initial contention (Hypothesis 1 – page 6 of the proposal) that introduction of terrestrial-derived iron (via runoff) may be identifiable in alteration of diatom assemblages. The PI takes considerable care in presenting the rationale for all his hypotheses (and correpsonding objectives), particularly within the context of the juxtaposition of snow melt runoff and coastal upwelling along a north-south gradient within coastal California. Moreover, the statement "the extension of these terrestrial and freshwater environmental changes in near-shore environments is poorly appreciated" is accurate and should form the theoretical basis for a sound (and well-funded !) coastal program. I also applaud the PI for a well-prepared and well-presented proposal (the proposal was relatively easy to read and understand).**
Rating	**very good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The entire proposal is based on (the presumption of) iron limitation within California coastal waters throughout the last several centuries and the subsequent introduction of iron within surface flows derived from Sierra Nevada snow melt. The PI has proposed a study justified (based on) current knowledge concenring coastal phytoplankton structure and function, upwelling, and nutrient dynamics. However, without a citation of previous studies presenting evidence for and/or inclusion of data indicating the introduction of iron−enriched riverine flows '…beyond the Golden Gate into the near−shore environment…', any attributing alteration in diatom assemblages within sediment cores to fluctuations in iron is conjecture. Moreover, delineating iron−/nutrient−induced alterations in assemblages from alterations potentially arising from prolonged regional/global climate change across decades also can only be inferred, not tested.**
Rating	**good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**Based on the hypoteheses, the PI has proposed an ambitous work. If one accepts the rationale behind the study (see Justification comments), the proposed approach is feasable and likely to generate extensive data concenring diatom paleoecology within California coastal waters.**
Rating	**good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**I question whether the content of the project is consistent with the PIs previous experience and/or academic training.** **The PI indicates that diatom characterization will be done with assistance from John Barron (phycologist at USGS Menlo Park) and a technician (to be identified) within Barron's laboratory. Obviously, the success/failure of this project lies almost entirely with accurate identification/interpretation of the diatom assemblages within the sediment cores. It is here that I have my greatest concern − I question whether characterization to the appropriate phylogenetic level (to the species level) can be completed.** **Based on information presented within the attached resume, the PI does not appear to possess any previous experience and/or skill set with diatom characterization (his sole listed publication is within a plant molecular journal). A perusal of Barron's publications within annual listings of USGS publications since 1997 (available at http://pubs.usgs.gov/publications/index.shtml) indicates a paucity of peer−reviewed manuscripts (mostly in 1997−1998) addressing coastal diatom**

	paleoecology and stratigraphy. The majority of Barron's contributions in this area are included within 'Proceedings of Ocean Drilling', with his more recent contributions (since 2002) addressing geological processes as they relate to global climate change. Further web searches failed to uncover any significant taxonomic work in any recognized diatom– and/or phycological–oriented journals. **Please be assured that I am only questioning whether the required skill set for successful completion of the proposed project exists, based on the information presented within the proposal. I am very familiar with the skills and experience required for diatom identification and enumeration (having been trained –and published – in this area). For someone to say that this type of work will be completed, without the required scientific skills presented in support of the planned work, does not make a strong proposal. If Barron does intend to take on the 'bulk of the work' for characterizing diatom assemblages, then he should probably be listed as a co–PI.** **Moreover, the PI indicates that diatom characterization will be based on image acquisition and recognition technology, following that presented by 'Automated Diatom Indentification and Characterization' technology – does this mean that such automated image processing exists within Barron's laboratory ? (only a reference to a contract concerning this technology and funded by the European MAST program is cited). If so, no previous experience with automated image–recognition methodologies and/or reliability of this methodology for characterizing California coastal diatoms assemblages is presented (in the form of citations, etc.).**
Rating	**fair**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**The PI will utilize previously−collected sediment cores; in addition, new cores will be collected at sampling sites for which sediment cores do not exist. The PI has extensively outlined plans for utilizing a comeercial vendor for acquriing sediment cores.**
Rating	**very good**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**If the project is successful, I would envison extensive (peer−reviwed) journal manuscripts/book chapter and theroretical white papers concerning reource/water management, oceaographic processes and function,and global climate change.**
Rating	**excellent**

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**see Feasibility above**
Rating	**fair**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**I typically do not comment on (the scope or amounts of) budgets – only if it is reasonable or not. I did note that the PI exhaustively allocated his services across all years. From this, it appears that this project will be the base (if not all) of his salary support. Within this thought, the budget was reasonable. However, as I commented earlier – it is an ambitous project and one I would think might not be difficult to complete in entirety within the proposed project/budget term.**
Rating	**good**

Overall

Provide a brief explanation of your summary rating.

Comments	**I have significant questions concerning the proposal rationale and (apparent) lack of previous diatom characterization/interpretation experience by the PI. However, if these particular tiems are accounted for (by the committee), I envision the proposal "Historic Contribution of Sierra Nevada Snow Pack Runoff to Iron Limited Upwelling Events off the Golden Gate" to produce information relevant to reource/water management, interpretation of oceaographic processes and function,and a greater understanding of meso-scale responses to global climate alterations.**
Rating	**good**